

UNIVERSITY OF LONDON
FRANCIS GALTON LABORATORY FOR NATIONAL EUGENICS

EUGENICS LABORATORY MEMOIRS. XIII.

A SECOND STUDY OF THE INFLUENCE OF
PARENTAL ALCOHOLISM ON THE
PHYSIQUE AND ABILITY OF
THE OFFSPRING

BEING A REPLY TO CERTAIN MEDICAL CRITICS OF
THE FIRST MEMOIR AND AN EXAMINATION OF
THE REBUTTING EVIDENCE CITED BY THEM

BY
KARL PEARSON, F.R.S.
AND
ETHEL M. ELDERTON,
GALTON RESEARCH SCHOLAR

WITH 3 DIAGRAMS IN THE TEXT

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**WORKS ON ALCOHOLISM BY MEMBERS OF THE GALTON
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PREFATORY NOTE

THE following reply to certain medical critics of our first memoir, accompanied by an examination of the rebutting evidence cited by them, is issued with no delight in controversy. The staff of the Galton Laboratory has far too much urgent work in hand—work which its members think of great social value, and with the magnitude of which their present resources are quite unable to cope—to take pleasure in demonstrating the fallacies of their critics. But we look upon the problem of alcohol as a great national problem, and a problem with which the nation can only hope to cope when it knows the actual facts as to alcohol. These facts are not known at present; they are obscured by passionate rhetoric and hopeless bias. Taking the *extreme* cases of alcoholism, cases where at least we have certainty of its evil effects on the individual—which is far more than we can assert of its customary and moderate use—we find alcoholism associated in the individual with mental defect. This *association* has been everywhere interpreted by the advocates of temperance as *causation*. They have never investigated whether the mental defect was antecedent to or consequent on the alcoholism, or how far it was partly one or partly the other. The family histories collected during some years in the Galton Laboratory as well as masses of other data seemed to indicate definitely that *extreme* alcoholism was only consequent on the pre-existing degeneracy of the stock, it was not in itself an antecedent to such defectiveness. The arguments produced by medical temperance writers to show that alcoholism was the source of defectiveness in the offspring were found to be based on *selected* families, and internal evidence in the data given satisfied us that these families were actually degenerate stocks. To those who have studied the heredity of physical and mental defects and noted the frequent appearance of alcoholism in such stocks, it must appear the height of absurdity to attribute deaf-mutism, dwarfism and physical deformity to parental alcoholism. And yet it is impossible to trace a long pedigree of deaf-mutism or of split hand and foot without coming across many associated cases of alcoholism. It is precisely so in the matter of albinism; it is quite easy to pick out pedigrees closely associated with imbecility and extreme alcoholism. If extreme alcoholism therefore be, as we believe from our data, a consequent and not an antecedent of defectiveness, then of what service for eugenic purposes can be a campaign which confuses all grades of alcohol users, and which would not reach the root of the matter, if it succeeded in cutting off entirely all opportunities for the procuring of alcohol? The problem in its truly national form, is the fundamental

problem of restrictive eugenics; it is summed up in the words: "Endeavour to cut off at its source the production of degenerate stocks." One step only in this direction—the segregation of the mentally defective—would affect at least 50% of the persons who ultimately find their way into prison, asylum, and inebriate reformatory. Thus our controversy with the medical temperance writers is not a personal one, it has a very close bearing on the most urgent problem of social reform. The question of whether extreme alcoholism is consequent on or antecedent to mental defect is a fundamental question at the present day. It is being obscured by the vague and unproven assertions of our opponents that every glass of beer, every drop of alcohol which the parent consumes produces its quantum of stupidity in the offspring. If therefore we appear to those, who run but do not read, to be opposing what they term a great social crusade, it is because we think, and shall continue to think, until substantial evidence is brought to the contrary, that alcohol is in its pernicious forms consequent on and not antecedent to mental defect, and that the rational solution of the alcohol problem must be reached by attacking mental defect and not by abusing unprejudiced students of the subject.

E. M. E.

K. P.

A Second Study of the Influence of Parental Alcoholism on the Physique and Ability of the Offspring

Much of the literature of alcoholism is of a rhetorical rather than scientific order, and the sooner scientific order and method are introduced the better.—*Sir Clifford Allbutt.*

(1) *General Reply to the Medical Critics of the First Eugenics Laboratory Memoir on Parental Alcoholism.* It is not an easy task to reply to the long series of misinterpretations and positive misstatements with regard to the conclusions of the first Eugenics Laboratory memoir on alcohol, which have been issued by the temperance press. The authors have been accused of every scientific blunder and of most social and moral delinquencies. That such would be the attitude of many individuals, who believe that their particular method of solving the social difficulties attending the excessive use of alcohol must be pushed either by true or false evidence we fully anticipated, and we gave expression to our anticipation in the concluding paragraph of our first paper.

Our own position was very fully defined beforehand. We realised the question of intemperance as one of the chief problems of our national life, and believed it to be one that essentially came under the definition of National Eugenics which alone controls the work of this Laboratory*. But, as in other cases, we were resolved to test these alcoholic problems for ourselves, by our own methods, and on material which had not been collected by any *ex parte* recorders already pledged to definite opinions. Our memoir was the first of a series of which the third is now at press, and may, though we would hope will not, provide further material for the vituperation and invective which has followed the first issue.

We propose in the present memoir to analyse some of the criticisms that have been made of the material dealt with in our paper, and to examine the weight of some of the rebutting evidence which has been brought forward by our critics. Of the many misstatements made with regard to ourselves only two must be corrected here, because they have some importance with regard to the Galton Laboratory for National Eugenics. Two of the most vigorous temperance advocates, Sir Victor Horsley and Dr C. W. Saleeby, alike in that they make very dogmatic assertions without either sufficient knowledge or reasonable criticism of their sources of information, have chosen to make very definite statements with regard to this Laboratory. They are absurd to any one with knowledge of the facts, but they do harm to the reputation of the Laboratory in quarters where

* "National Eugenics is the study of agencies under social control that may improve or impair the racial qualities of future generations, either physically or mentally." This definition was adopted as a guide to the work of the Laboratory on its foundation in 1907, when the older "Eugenics Office" was replaced by the Eugenics Laboratory.

those facts are not known. Sir Victor Horsley on July 28th, 1910, at a breakfast of the National Temperance League, said of the first memoir that it "is a most unhappy one; and it has been disowned by the Chairman of the Eugenics Society, for which Miss Elderton and Professor Pearson are working." This statement was at once contradicted in the *British Medical Journal* (August 13, 1910). The Eugenics Laboratory is under the control of the University of London; it has no relation whatever to the Eugenics Education Society, of which the authors are not even members, and the sole link between the two lies in the fact that our Founder is Honorary President of the Society. Notwithstanding this contradiction, which could hardly escape Sir Victor Horsley's attention, he has allowed the statement to be repeated under his name in the *National Temperance Quarterly* for September (p. 144). The second misstatement to which it is needful to refer occurs in a paper by Dr C. W. Saleeby in the *British Journal of Inebriety*, Vol. VIII. p. 54, and runs as follows: "When he founded the Eugenics Laboratory, now several years ago, Mr Galton, as he was then, asked the present writer and one or two others to meet the staff of the Laboratory once a month, but only two or three such meetings were sufficient to show more than one of us that no good was to be done. If the present biometricians were biologists, as their founder was," then, Dr Saleeby tells us, their patience would be of service and they would accept medical aid*. The Eugenics Laboratory came into existence and was first called by its present name in 1907, when the present Director took up the reins; it was then for the first time associated with the Biometric Laboratory at University College and its official publications were then first started. The present staff of the Laboratory did not come into existence till 1907, and as Dr Saleeby must know perfectly well, he has never met the "present biometricians" in committee or council of any sort connected with the Laboratory; nor has he had at any time a voice in its councils; it would have entirely precluded the present staff from joining it had he been in any way associated with its activities. The old "Eugenics Record Office," which existed before 1907 had a biologist as its research fellow and the chairman at the gathering to which Dr Saleeby refers was, we understand, a biologist also. Dr Saleeby by a *suggestio falsi* has led the public to suppose that the present Laboratory was at one time aided by an advisory committee of which he was a member and which would have guided, but for their perversity, the "present biometricians." Thus Dr Basil Price (*National Temperance Quarterly*, Vol. VIII. p. 176) talks about "the committee originally formed to supervise the Galton Laboratory Reports." Men of science are not in the habit of submitting their researches for supervision to committees of the kind suggested by Dr Saleeby and Sir Victor Horsley, and the sole responsibility for publication in the case of the Eugenics Laboratory has always rested with the Director, as the

* Dr Saleeby states (*British Journal of Inebriety*, Vol. VIII. p. 53) that he abandoned medical practice in 1902 to devote himself to the study and advocacy of eugenics. His first qualifying medical degree dates from 1901.

responsibility for ultimate choice of subjects has rested with the individual workers. This does not connote that the Laboratory rejects medical aid; there has never been a time since its present organisation, which dates from 1907, when it has not sought and obtained, whether dealing with alcoholism, insanity, tuberculosis, eyesight, or heredity of disease, the help of the medical profession. This help has been always sought for and has been given most generously, and one of the humours of the situation has been to find the more talkative but far less authoritative members of that profession criticising statements made in our memoirs as those of "mere mathematicians" when these had actually received the sanction of men of far greater weight in their own faculty! It is perfectly open to Sir Victor Horsley and his comrade in arms Dr Saleeby to condemn to their hearts' content the publications of this Laboratory; that they make statements with regard to its constitution which are misleading or wholly false exceeds the limits of legitimate criticism. From Sir Victor Horsley we had anticipated immediate and frank acknowledgment of his error.

If we turn to our critics they naturally divide themselves into three classes. In the first place we have the paid officials, or platform orators of various temperance organisations; their criticisms have no scientific value and will not be noticed here. In the next place we have two or three economists, who have attacked the memoir on the ground that the populations dealt with were not fair samples of the working class population. They have been fully dealt with in a *Supplement* to the original memoir issued in September*. They will only be incidentally mentioned in this paper. It is a noteworthy fact that Sir Thomas P. Whittaker, with that *Supplement* before him, restates the arguments of these economists without one word of reference to the rejoinders made to them! Until the replies made to the economists have been met, it is idle to repeat them here. The question therefore of the representative character of our samples will only be referred to incidentally. Lastly we have the third class of critics, those with special medical training who have written on the subject of alcohol and profess to have studied its literature. It is this class of critics to whom we shall devote our present space.

Now the characteristic feature of the English literature of alcohol is the total absence of any original investigation on the point we had under discussion, namely the influence of parental alcoholism on the offspring. Our critics repeat each other and cite two or three well-known continental and American authorities, Demme, Bezzola, Laitinen and MacNicholl. In the bulk of these cases they give no reference whatever to the locus of the original papers and there is more than a shrewd suspicion that they have never read them. What is quite certain is that they have never examined them with a view to ascertaining whether the originals are statistically self-consistent. They talk about the "weighty statement" of Demme, about the "careful evidence" of MacNicholl or the "well-

* *Supplement to the Memoir entitled: The Influence of Parental Alcoholism on the Physique and Ability of the Offspring.* Questions of the Day and the Fray, No. 1. Dulau and Co., Soho Square.

known work" of Bezzola. Or again, these gentlemen cite each other: "Only an insignificant number of drinkers' children are physically sound, Sir Victor Horsley tells us," writes Dr Johnston*, but he gives us no reference to the book and page, and Sir Victor Horsley's writings may be sought in vain for any *independent* statistics. "The child of the alcoholic mother is often dying before it is born. Five times as many alcoholised infants die as those of sober mothers," says Mr Pearce Gould. But we are given no reference where we can verify the statistics upon which these statements are based. Or again, "Dr Saleeby has told us that when formerly working in the slums of Edinburgh and York, he attended the birth of children who were born drunk." We ask in vain for a reference to the original paper, that we may duly appreciate the signs by which this clinician diagnosed drunkenness in the newly-born baby. These things may be true or they may be false, their reiteration by one writer on temperance after another without any examination of the original data is one of the signs of how thoroughly unscientific is and has been the English discussion of this vital problem.

We propose to illustrate this by first stating the sort of criticisms that have been made of our memoir, and then applying them to the material which these medical temperance authorities cite as demonstrating that our data are valueless.

(2) *MacNicholl's Data*. We found in Edinburgh that 54% of the fathers drank, *i.e.* took more alcohol than the social workers, police and their employers considered good for them†; 24% of the fathers of the mentally defective children drank in the case of Manchester. In the supplement to our memoir we indicate that data are forthcoming to show that 20% to 40% of the workers in the Lancashire towns are "drinkers" in the sense in which we have applied the term in the Edinburgh data. Now what are the criticisms made of these results?

(i) That not a layman but only a medical man can determine whether a person is alcoholic or not.

(ii) That the proportion of 25% of alcoholic parents in the population cannot possibly be correct, and that accordingly our data apply only to a degenerate population, and are unreliable‡.

Now let us apply these results to Dr MacNicholl's American data which are quoted with high approval by Sir Victor Horsley (*Alcohol and the Human Body*, 1907, p. 325), Dr Saleeby (*British Journal of Inebriety*, Vol. VIII. p. 63), Dr Basil Price (*Ibid.* p. 72, from Horsley), Dr Claude Taylor (*National Tem-*

* *Alcohol, the Parent and Child*, First "Hicks" Counties Lecture, 1910.

† Sir Thomas P. Whittaker converts this into the statement that the fathers were known or suspected to be "drunken." We in our memoir used no such classification of "drunken."

‡ Even if the populations dealt with were the lower working classes, it would not affect the argument, as we have shewn in our *Supplement*. It is the *differential* influence of alcohol we are concerned with, and it is as reasonable to argue from its differential effect on a special class of man to all men, as to argue from its differential effect on mice or guinea-pigs to what occurs in the case of man.

perance *Quarterly*, September, 1910, p. 183, who again cites Horsley) and Dr T. Johnston (*Alcohol, the Parent and Child*, 1910, p. 10), who gives no reference at all.

We turn to Dr MacNicholl's paper. We find he reports on 55,000 school children of whom 10,790 were females and 44,210 males. He reports that he could not get as many details as he should have liked owing to the "numerous duties of the school teachers." In other words his analysis of the parental attitude to alcoholic drinks which was reported in 20,147 cases was a report not provided by a medical examination of the parents, but by laymen and these not trained social workers as in the case of our Edinburgh data. The trustworthy character of the records is therefore far less than that of our Edinburgh data, and much less than that of the Manchester data, where one parent was examined by a highly qualified medical man, and *the home visited* by a qualified social worker*.

In the next place the parents were only divided into *two* classes, *drinking* parents and *abstaining* parents. Will Sir Victor Horsley and Dr Basil Price assert that those *abstaining* parents were *total abstainers*? If so, they must be prepared to substantiate the fact that 67% of an American population which itself contained 32% of foreigners were total abstainers. There is, we think, small doubt that this 67% of abstainers, as reported by the school teachers, covers precisely the same class as we have called "sober" in the Edinburgh returns. In other words, Dr MacNicholl's classification of "drinking" and "abstaining" is precisely the "temperate" and "intemperate" of our Manchester (or Edinburgh) classification. And yet the very men who cite MacNicholl's data with the highest approval tell us that such broad classifications as we have used render our results futile! But note further what happens: there are 33% of drinking parents! This is exactly the percentage which our critics find marks a degenerate population, which we had no right to take as a sample to test the problem of alcoholism upon†! But we have not done yet! Not only have we been told that our classification was made by laymen, and that our samples represent a degenerate population with an enormous percentage of drinkers, but a third condition has been laid down as an essential for reaching any result of value. Here is what Sir Victor Horsley writes: "The enquiry purports to differentiate between alcoholic and non-alcoholic parentage, yet as Dr Sullivan points out, there is no indication as to whether the alcoholism had set in before the offspring were born! When children aged 14 are being investigated we require to know the habits of the parents fifteen or more years previously! Yet this simple precaution has been entirely overlooked" (*National Temperance Quarterly*, September, 1910, p. 181). Dr Basil Price writes: "No inquiry apparently was instituted into the habits of the parents *previous to* and *during* the conception of the children; a factor of every importance, the omission of which quite vitiates

* Dr MacNicholl (*Medical Temperance Review*, August, 1905, p. 246) gives no evidence whatever of home visitation or medical examination of the parents.

† Cf. Dr Keynes, who finds 25% of alcoholic parents impossible in a working class population.

the value of the report apart from other faults" (*Ibid.* p. 177). Their colleague Dr Saleeby writes that we have dealt with the influence of parental alcoholism on the physique and ability "*without troubling to inquire in a single case whether the alcoholism or the offspring came first*" (*British Journal of Inebriety*, Vol. VIII. p. 59).

Now let us assume for a moment this criticism to be a valid one, then what are we to think of the logical acumen of Sir Victor Horsley, who cites MacNicholl's results as of striking value? What must we hold to be the scientific weight of Dr Basil Price, who uses the MacNicholl data to rebut our conclusions? Or, lastly, of Dr Saleeby's rhetorical judgments, who quotes them as being authoritative? Either these writers have never studied MacNicholl's paper, or they find it sufficient to use a process when it supports their own conclusions, and then call it discredited when used to deduce results with which they are not in agreement.

Now every word that Sir Victor Horsley, in association with Dr Saleeby and Dr Basil Price, has applied to our method and memoir on this point applies with greater force to Dr MacNicholl's material. He has neglected to inquire whether the alcoholism or the offspring came first. He has not even provided the data which we gave to enable the importance of this knowledge to be tested, for he has not provided the ages of the children discussed by him. The illustration we have given here of the acumen, the logic and, we will venture to say, the fair play of these temperance medical critics is not unique, as we shall soon see. But in the face of it how can those sociological workers who wish to get at the truth trust any conclusions formed by such logicians? The intellect of man can be affected by toxicants more subtle than alcohol, and the most dangerous of these is the passion for collecting, without weighing, any statements which will support a prejudgment.

The actual answer to the difficulty raised by Sir Victor Horsley and Dr Saleeby could have been found by them at once had they studied the data in our memoir. Of the alcoholic parents a slightly *larger* percentage of the offspring falls between 5 and 7 years of age than in the case of the sober parents, 30% as against 27% and the ratio of the number of children between 5 and 7 to those between 12 and 14 is for *both* drinking and sober parents 1.6. But if the parents only became alcoholic to any sensible extent after the birth of their children*, it will be clear that the ratio of young to old offspring in the case of the alcoholic must be very different from what it is in the case of the sober parents. Actually the alcoholic have as many *young* children as the sober, and there is no place for a large number of the alcoholic to have become such after the birth of their children. The only artifice by which Sir Victor Horsley and Dr Saleeby could get out of this difficulty would be to assert that alcoholism never develops until the youngest children of a family are of school age. Granted this, however, we see that the assertion that alcoholism in the parent produces mental defect in the children must be idle, for all the children would be born before alcoholism begins!

* There is doubtless *some* development of alcoholism after the birth of the offspring, but the above statistics go a long way to show that it is not a widely spread tendency. The drinking parents have relatively as many *young* children as the sober.

But let us return to Dr MacNicholl, although we hardly know whether our medical critics will now retain or reject him as an authority*.

In mental defectiveness the following categories have been recognised: (i) the certifiably insane; (ii) very defective, congenital imbeciles, degenerates and epileptics; (iii) defective, eccentric, silly, dull, senile, or subject to periodical paroxysms of ungovernable temper probably of an epileptic character; (iv) backward, generally incapable of average attainments in more abstract matters and much behind in school work; (v) all those of normal intelligence. Of these divisions, (i), (ii) and (iii) cover what are termed the mentally defective in the scholastic sense. In London they amount to a little over 1%, certainly not 2%. Class (iv) covers probably about 5% to 10% of the children. Now Dr Basil Price talks about Dr MacNicholl studying mental deficiency in school children and speaks of the effect of parental inebriety on the production of the *feeble-minded* (the italics are his). Sir Victor Horsley tells us that 55,000 children were examined by Dr MacNicholl and that he found 17% actual "dullards," 25% "very deficient" and 16% "deficient." This is not exactly Dr MacNicholl's own statement; he says that 25% were "very deficient," 17% "dullards" and 16% "below standard†." Only 42% were "standard" or of normal intelligence. Now nothing like this has ever been our experience in dealing with large quantities of English school data. Dr MacNicholl does not define his terms, but presumably "below standard" roughly corresponds to our class (iv), which contains 8% to 10% of children. Below this MacNicholl places 25% of "very deficient" and 17% of "dullards," as against the less than 2% of such cases found in England. Is it not obvious that we are dealing with either a wholly exceptional population or with an estimation of mental deficiency which is entirely fallacious? Among our 1092 Edinburgh children 29 were "defective" and 170 "dull," or 2.7% and 15.8% respectively. These are large values compared to the London 2% and 10%. But what do the critics who assert that our Edinburgh population was not a fair sample and could not be used to demonstrate anything say when they have a population 25% deficient and 33% dull? Why, they accept it without comment and use it as a normal population to discuss the influence of parental alcohol on the offspring! It is impossible to believe that they have ever examined Dr MacNicholl's paper.

Again Professor Marshall, Dr Keynes and Sir T. P. Whittaker have all alike asserted that the 30% of drinkers in Manchester and 54% in Edinburgh were

* Will they apply to Dr MacNicholl the criticism of their own great authority, Sir T. P. Whittaker, *i.e.* "the attempt to determine the influence of parental alcoholism on the physique and ability of the offspring, when he does not know whether the parents upon whose present habits he bases his elaborate calculations and extraordinary conclusions, were sober or drunken in the years prior to and at the time the children were begotten, is an absurdity. It is to erect a pretentious but entirely fallacious argument upon an absolutely rotten foundation"?

† In an authoritative paper Dr Luther H. Gulick (*Medical Record*, New York, July 28, 1906, p. 125) states that "there is a small percentage of children—from one-half of one to two per cent.—so below grade mentally as to be incapable of the most profitable education in classes with average children." These numbers agree with European experience, but not with Dr MacNicholl's extraordinary figures.

incompatible with the deduction of any results bearing on the problem of alcoholism. Yet Dr MacNicholl obtained a family history of 3711 children and found in this case that 73% of the parents drank! His material, however, is good enough to be used by Sir Victor Horsley and Dr Saleeby as evidence of the superiority of sobriety. Nay, not only in this population of children were 34% child-drinkers, but in the 55,000 previously referred to 27% were child-drinkers. In the schools with which Dr MacNicholl is dealing he tells us of children staggering drunk into the schoolroom, or of children by the ten being found in a state of intoxication, of boys going about with beer cards which were stamped with a hole each time they got a glass of beer, the boys with the most holes per month getting prizes. This is the population upon which Sir Victor Horsley thinks it suitable to test social problems! We should like to have Professor Marshall's views with regard to it. The population of 3711 children with 73% of drinking parents, 34% of drinking children, 81% of "dullards" and 64% of "neurosis or organic disease" is treated by Sir Victor Horsley and Dr Basil Price as a fair sample of a population upon which the evils of parental alcoholism can be measured, while our Edinburgh and Manchester data with nothing like these signs of general degeneracy is said by such critics to be irrelevant. Neither Sir Victor Horsley nor Dr Basil Price draws anywhere attention to this extraordinary state of affairs; they quote Dr MacNicholl with complacency and this although he has nowhere in his paper given us the information by which we could discriminate *whether the drinking of the parents, or the beer and spirit drinking of the children themselves was the source of the extraordinary amount of degeneracy found in the child population*. Sir Victor Horsley terms these children "ordinary children" (*Alcohol and the Human Body*, p. 324), and Dr Basil Price* without any comment cites these data as illustrating the effects of parental inebriety on the offspring. In a further paper ("Alcohol and the Disabilities of School Children," *The Journal of the American Medical Association*, Vol. 48, pp. 396—8, Chicago, 1907) Dr MacNicholl gives still more surprising results. He tells us that in some schools less than 20% and in some classes less than 2% of the children were found absolutely normal in body and mind. An examination of the records of 63,000 school children in 150 schools and 1749 classes showed 25% very deficient and 58% "below standard." Of 10,000 children in city schools 35% had diseases of the heart, 27% were tuberculous, 60% anaemic and 80% suffered from some neurosis, *i.e.* each child had on an average $2\frac{1}{4}$ defects. So common, he tells us, are organic and functional disorders in New York city that should those afflicted be excluded two-thirds of the schools would be compelled to close their doors. He divided the children into

* Dr Basil Price complains of the "looseness" of our data when we accept Dr Ashby's "temperate" and "intemperate" as a classification of the Manchester population. Yet he accepts without a murmur MacNicholl's "drinking" and "abstaining," although the former might contain everybody, from one drinking a glass of beer a year to the chronic inebriate! In no single word does he question such vague terms, used by MacNicholl without any verbal definition, as "dullard," "standard," "below standard" and so forth.

two classes (a) and (b), the division being between "good or prosperous" and "poor" circumstances. In (a) 30% of the boys smoke, in (b) 80% of the boys smoke. In considering the habits of drinking among the *children* he reports in 34,000 cases of (a) 73% abstainers, 23% drinkers of beer, drinkers of spirits 4%, drinkers of beer and spirits 12%. In 6879 cases of (b) there were 50% abstainers, 43% beer drinkers, 7% spirit drinkers and 40% drinkers of both beer and spirits. In class (a), 32% have drinking parents, 68% abstaining; in class (b) 85% have drinking parents, 15% abstaining. It will be interesting to hear what the critics of our Edinburgh population with 54% of drinkers say to a population with 85% of drinking parents, which is the condition of nearly one-sixth of the children examined in New York, the "good or prosperous" section having 32% of drinking parents—our Manchester percentage. Will they now discard MacNicholl's work from the position it has been given in their writings? Will they also demand from him an inquiry as to whether the drinking and smoking of the children was not as much a cause as the drinking of their parents of their mental deficiency? *Will they also ask him why in no single instance in this material he has distinguished between the cases in which either one parent only or both parents drank?*

We may stay to consider here a further paper by Dr MacNicholl, which no doubt will ultimately meet with the approval of our medical temperance critics. It was read at the Eleventh International Congress on Alcoholism. It is published in the *Medical Temperance Review*, Vol. XII. pp. 53—6, 1909. Dr MacNicholl professes to have traced two hundred families, 100 with great grandparents in moderate and 100 with great grandparents in prosperous circumstances, through four generations and representing 35,266 individuals. The information given is so scanty that it is impossible to understand what Dr MacNicholl has really done. He tells us that "each and all of the great grandparents are classified as moderate drinkers, that is they were never intoxicated, but drank malt or spirituous liquors at regular intervals with or without their meals." As Dr MacNicholl tells us of the diseases (brain and nervous system, heart and lungs, liver, cancer, Bright's, etc.) from which each generation suffered, he must have knowledge of the last generation for at least 40 to 50 years; if we add two more generations to this, Dr MacNicholl's information must cover 90 to 100 years. Now it must be remembered that if we start with a family of 5, they will have 8 great grandparents; these four couples would give on an average 20 children, and one child of each of these families would go to make up the two couples who formed the grandparents; the two couples would produce on an average 5 children each. Thus on an average estimate a family of four generations consists of $8 + 20 + 10 + 5 = 43$ individuals. Dr MacNicholl has an average of 176 individuals, four times as many, in each of his families. He can only have obtained such numbers *by including cousins*. But on the average every five cousins he introduces means an additional four great grandparents. We must therefore understand that Dr MacNicholl is dealing with, not 8 but possibly 20 or 32 great grandparents, living nearly a century ago. These great grandparents for the 200 families would themselves provide a fairly big task, when we require to demonstrate, as Dr MacNicholl has done,

that (i) "each and all" were moderate drinkers and never intoxicated, (ii) that all were healthy and vigorous—another of Dr MacNicholl's conditions—and (iii) that they had no "indication of any organic or functional disease," for that excluded a family from the list. If Dr MacNicholl knows such facts for each of 35,266 persons, he has the greatest wealth of family pedigrees of any man living. We in the Eugenics Laboratory know the extreme—almost insurmountable—difficulty of getting this information for even *three* generations. And in view of this we call for the publication of those 200 pedigrees with detailed information as to the physical defects, chronic diseases and drinking habits of over 35,000 individuals. If they really are what they profess to be, this Laboratory is willing to publish them in Dr MacNicholl's name to-morrow.

But it is time to give Dr MacNicholl's conclusions. Starting with "each and all of the grandparents moderate drinkers," he provides the following data :

Great Grandparents in Moderate Circumstances.

	1st Gen.	2nd Gen.	3rd Gen.	4th Gen.
Moderate Drinkers.....	100 %	41 %	22 %	38 %
Heavy Drinkers.....	0 %	18 %	10 %	4 %
Abstainers	0 %	40 %	67 %	57 %

Great Grandparents in Prosperous Circumstances.

	1st Gen.	2nd Gen.	3rd Gen.	4th Gen.
Moderate Drinkers.....	100 %	61 %	50 %	27 %
Heavy Drinkers.....	0 %	8 %	10 %	5 %
Abstainers	0 %	30 %	39 %	67 %

Now we have a marked change here in the percentages of drinkers, we start with no abstainers and reach 57 % to 67 %. The stocks are becoming less and less alcohol drinkers.

Now look at the result.

Physical Defectives.

Great grandparents	1st Gen.	2nd Gen.	3rd Gen.	4th Gen.
Moderate Circumstances.....	0 %	80 %	87 %	92 %
Prosperous Circumstances ...	0 %	71 %	87 %	96 %

The chronic diseases show little regularity and hardly fluctuate beyond their probable error round 75 % for generations 2, 3 and 4. Now here is a section of the very investigation Sir Victor Horsley has demanded (see p. 26 ftn.), an inquiry into the result of drinking during four generations, and what do we find? Why, that while abstainers form an increasing percentage in the 200 families, until they now, in the last generation, number 62 % of the whole, the number of physical defectives has increased to such an extent that now only 6 %* of the fourth generation of these 200 families is without such defectiveness! We start with "each and all the grandparents" (a hundred years ago!) drinking, but without physical defect, we conclude with 62 % abstainers but with only 6 % free from physical defects. Surely the scoffers will say, What profits it to abstain, if only 6 % of our

* Dr MacNicholl says 5 %.

children are normal? Not so Dr MacNicholl; he notes "the alarming harvest of abnormalities in the offspring, each generation showing more and more degeneration and greater susceptibility to disease," and puts his results for the two sets in the form

	1st Gen.	2nd Gen.	3rd Gen.	4th Gen.
Normal.....	100 %	18 %	12 %	5 %

and concludes by the suggestion that "to the moderate drinker belongs no small share of responsibility for the mental and physical disabilities which now afflict the race." Now there is hardly a member of the professional and upper classes in this country to-day whose great grandparents were not "moderate drinkers," and probably at least their grandparents also. Will any one assert that all but 5 % of those classes are degenerates or suffer from some physical defect? Yet all these ancestors according to Dr MacNicholl have constituted themselves possible progenitors and ancestors of a race that will "produce a harvest of victims for hospital, almshouse, insane asylum, and gaol. Once mental or physical deformities come within the grasp of this ubiquitous law, they transmit themselves to posterity with unerring facility" (p. 53). Under the circumstances why advocate abstinence? It seems to have been quite hopeless in its struggle against the "ubiquitous law" set going by these healthy and vigorous grandparents without chronic functional or organic disease, who condemned us by their moderate drinking to be only to the extent of 5 % normal human beings.

To sum up, it can only be an exceptional English family whose great grandparents did not "each and all" drink moderately. The fact that 95 % of physical defectives and degenerates do not exist among us is sufficient to show the complete futility of such statistics. Even if they were true, there would be as much logic in attributing the 95 % of defectives to the increasing abstinence as to the "moderate drinking" of great grandparents; either is mere association, not demonstrated causation. Yet it is the material of such a writer which Sir Victor Horsley and Dr Saleeby hold worthy of scientific credence*!

(3) *Laitinen's Data.* The next illustrative example we will take is the paper by Professor Taav Laitinen of Helsingfors. This paper is published in the *Proceedings of the Twelfth International Congress on Alcoholism*, 1909, pp. 263—270. It is cited with great approval by Dr Saleeby and Sir Victor Horsley. Dr Saleeby terms Laitinen the "most patient and distinguished among living students of alcoholism." He states that the paper "surpasses in magnitude and precision all the many studies of this subject which have proved (!) the relation between drink and degeneracy."

Now Sir T. P. Whittaker accused the writers without a grain of evidence, and indeed without an atom of truth, of having "classed" as sober the families in which we only knew the relation of drink to *one* parent. Dr Saleeby, with his customary habit of talking before weighing evidence, cites Laitinen to support Sir T. P. Whittaker. The citation is apt, for Laitinen has done precisely what we did not do, but Sir T. P. Whittaker accused us of doing. He has *made no distinction*

* It is perhaps unnecessary to add that Dr MacNicholl again never informs us whether the drinking antedated or postdated the conception.

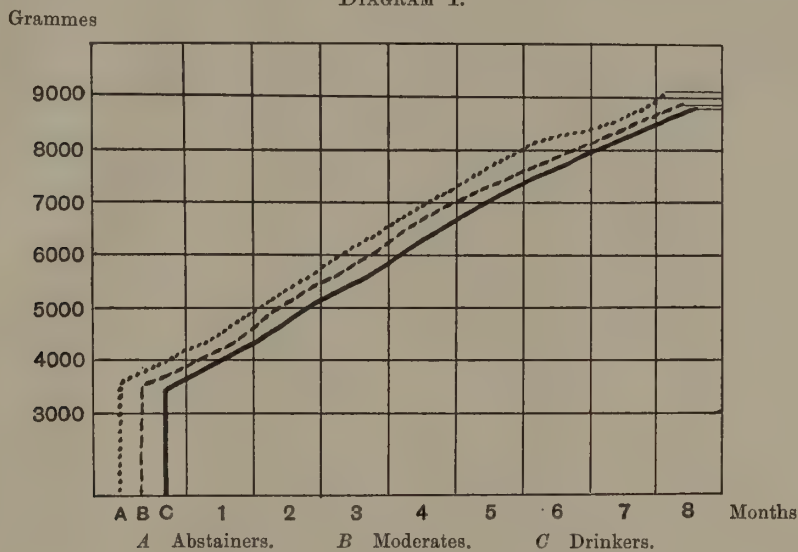
*between parents one of whom drank, or both of whom drank**. In our memoir, we showed exactly what Laitinen professes to show and what he is praised by Dr Saleeby for showing, namely that the weight of a child of an alcoholic parent was less than that of a sober parent. But because we distinguished between the cases of father and mother, which Laitinen fails to do, and because we measured the exact extent of the relationship, we were able to see that (i) the drinking of the mother had more influence than the drinking of the father, and (ii) that slight as was the effect since it influenced girls more than it influenced boys, it was unlikely to be due to toxic action on the foetus. We considered that possibly for school ages the influence on weight was due to another environmental factor; drinking mothers were more often employed than sober mothers, and that this threw more home duties on the girls of such mothers, and kept them more from play and exercise (*Memoir*, p. 7). But we kept our minds open: Edinburgh has not a very mixed population like that of Glasgow or Helsingfors, but we were prepared to learn that a racial difference existed between the more sober and more alcoholic classes (*Memoir*, p. 37). It is difficult to see where our results contradict those of Laitinen, who, we are told, has reached results "precisely opposed" to ours. Laitinen deals with three series of families, and each series is open to very serious objections. In the first case he issued 15,000 circulars—to medical men?—no, to the *parents* of new-born children, and he asked these parents to weigh their children for eight months and to say *themselves* whether they, the parents, were drinkers or abstainers. Of these circulars 2125 were returned, or *only* 14%. Thus laymen had to settle for themselves under which category they fell. Is it likely that the heavy drinker would fill in such a paper at all, still less weigh his baby once a month to provide, as he would naturally suspect, evidence for the well-known temperance professor on the subject of drink? We have been told that only a medical man can appreciate whether a person is alcoholic or not, yet when it suits their particular views our temperance critics will accept evidence to be provided by the drinker himself! Laitinen tells us that he terms "abstainer" a person who has either never taken alcohol or at least not since his marriage. By the term "moderate" he refers to a person who takes no more than one glass of beer a day and by a "drinker" he denotes a person who takes more than this. Now these may be his definitions, but in the circular—which he addresses thus: "Honourable Fellow-countrymen! You will do a great service to science if you will conscientiously fill in this circular respecting your new-born child during the first eight months of its life"—he merely asks if the parents are drinkers or abstainers and to state their daily consumption of beer and spirits. There is in the circular as he gives it no reference to the period during which they have drunk, or whether the period of abstinence has been during the marriage. This may have been ascertained in some other way, it is not referred to in the *questionnaire* as cited by Laitinen. Of this population, according to Laitinen, 60% were "more or less drinking." The children, however, are classed under the heading of children of

* Laitinen asked for the information in his schedule, but if the information was given he has not used it.

"abstainers," "moderates," "drinkers." In not a *single* case out of 2125* is it noted that one parent drank and the other abstained! If this does not occur in large numbers in Finland, that country must be wholly different from the rest of Europe, where the men drink far more frequently than the women. It would be interesting to know how many heavy drinkers got into a series of this kind, and where a sober mother with a drinking husband was placed!

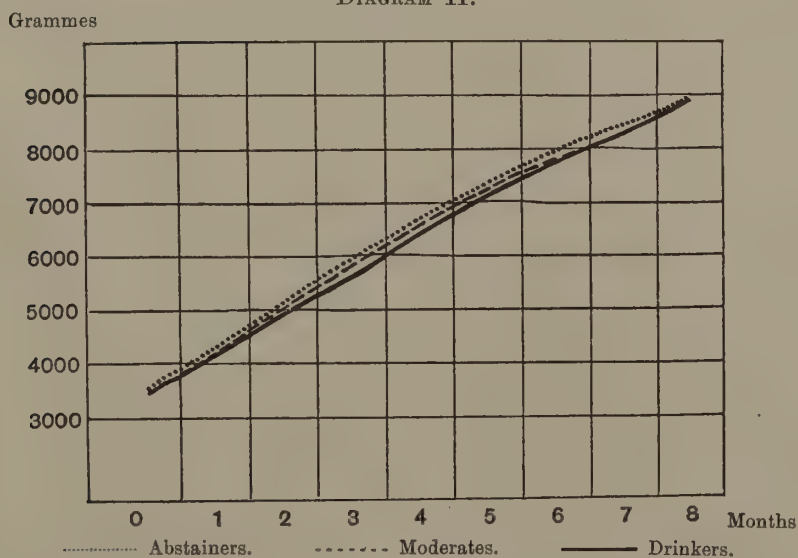
Laitinen gives tables of the average weights for eight months and graphs which are wholly deceptive and much exaggerate the apparent differences. This may not

DIAGRAM I.



After Prof. Laitinen, M.D., Helsingfors.

DIAGRAM II.



As Laitinen's Diagram would appear had he plotted his actual data.

* Nor indeed out of the later total of 5845.

be intentional, but this only demonstrates how wrong it is for those who have no statistical training to prepare statistical material. The fact is that the actual differences obtained are so insignificant on a diagram of the size used, that Professor Laitinen has thought it advisable to magnify them by the simple artifice of causing the baby of the abstainer to be born about two-thirds of a month earlier than the drinker's baby; the eye compares on each vertical ordinate the abstainer's baby at an age sensibly older than the drinker's baby!

Laitinen states his conclusion to be that the average weight of the abstainer's child is greater at birth, and that these children develop more rapidly during the first eight months than the moderate drinker's child, and the latter exceed in the same way the drinker's children. Now we think this statement is incorrect. It is a noteworthy fact that Laitinen finds a sexual difference between boys and girls, but that unlike our result for children of school age, where the weight of the girls was the more affected, he finds the difference in boys more significant than in girls. His numbers are somewhat erratic, probably due to vaccination and other troubles affecting the weight. But the following represents the measure of the difference:

Percentages by which Drinkers' Babies' Weights are less than those of Abstainers.

	When born	1 and 2 months old	7 and 8 months old
Girls.....	3.6 %.	4.4 %.	2.2 %.
Boys.....	4.4 %.	8.4 %.	7.2 %.

The "moderates" give somewhat less percentages in all cases. It is not correct to say that the abstainer's children develop more rapidly; the difference between the babies of abstainer and drinker is rather reduced in boys, and markedly reduced in girls. Laitinen also finds that the number of toothless children is greater at eight months with the drinkers; their children have one less tooth cut and the average age at first cutting is greater. All these results, equally visible in the children of the moderates (one glass or less of beer a day!), are attributed by Laitinen to the alcohol. He finds association, he asserts causation. Now he makes no attempt to measure any differences of race or environment in the two cases. He assumes them to be apparently of similar character. Yet when we remember that about 55 % of the population of Helsingfors is of Swedish descent and 34 % roughly Finnish, we think ourselves justified in asking that some statement as to the races investigated should be made. Weight of baby at birth is well known to be a racial character, and it is very unlikely that Swedes and Finns, and probably Russians, drink in the same proportions.

Laitinen states that the samples he has taken do not differ in the circumstances of the drinking and non-drinking groups. We can illustrate his measure of likeness of circumstances by his treatment of 59 drinking and 50 non-drinking families in a little country town, "where," he tells us, "the daily habits of the inhabitants are well known to everybody." The division here is merely between "abstainers" and "drinkers," and there is no evidence at all as to whether the drinking or conception came first, only "the daily habits of the inhabitants are well known to everybody."

The ages of the fathers and mothers show that they are not past the child-bearing age, hence to obtain a rough value we have deducted two years for each child from the present ages to reach age at marriage; this is rather considerable, but there are a certain number of miscarriages recorded, 1 % for abstainers and 6 % for drinkers.

	Gross size of Family	Age of Father	Age of Mother	No. of Rooms	Ages at Marriage	
					F.	M.
Abstainers ...	4.28	47.2	39.5	2.83	38.6	30.9
Drinkers.....	4.72	40.3	39.0	2.31	30.9	29.6

The ages at marriage are at once seen to be extravagantly high for any test sample of the population. The greater fertility of the drinkers (here again there is no distinction drawn between cases in which alcohol is taken by one or both parents, a *siné qua non* of any scientific inquiry !) is in agreement with our results. But we see a very marked differentiation again between the two classes, the father was only one year older than the mother in the case of drinkers, but *seven* years older in the case of abstainers; there was half a room, 25 % difference, in the average house accommodation of the two classes. Again we say that until these points have been allowed for, it is impossible to say whether the differences observed in the defectiveness of the children are environmental or hereditary—exactly as they appear to be in Demme's selection of 10 drinking and 10 sober families. That the statistics refer to a degenerate or slum population, if we are to trust another of our critics, Professor Marshall, is evidenced by the drinkers only occupying 2.3 rooms and the abstainers 2.8*! Yet the men who make these criticisms against us do not hesitate to use Laitinen's results for their own propaganda.

We now turn to Laitinen's third series of observations. Here again there is no distinction between those cases in which one or both parents were abstainers or drinkers. The number of children was investigated, and the average number runs according to the category from four to five. No evidence whatever is given as to how this series of "3611 families with a great number of children" was obtained. They must have been observed over a period of some 10 or more years, because the *average* number of children is five and the weights of all the children are given as babies. There is no statement whatever bearing on the point of the drinking having begun before the birth of the first child, and there are several features which cast grave doubt on the results as tabled. Thus from his 2125 schedules of new-born children he finds for the weights of the boys of "moderates" and "drinkers" at birth 3780 and 3700 grammes respectively. But when he deals with 5845 instead of 2125 cases he obtains precisely the same figures to the last place, 3780 and 3700 again. To anyone acquainted with statistical results, that two means based on 2125 and 5845 cases should agree to four figures is rather astonishing. But other surprises await us. In dealing with

* The average number of rooms in Laitinen's first or schedule population are: abstainers 3.73, moderates 3.95, drinkers 3.70. This shows that his two series belong to wholly different social classes!

the 3611 cases in which he knows the history of *all* the children he mixes them up with the 2125 schedule children, and then says the number of children is misleading relative to the number of families. In other words he appears to have added 840 isolated children into the abstainers' children, 623 into the moderates', and 662 into the drunkards' children (see his Table II, p. 267, and statement on p. 264; the numbers in the third paragraph of p. 269 are erroneous). But he says if we subtract these results we shall find that the number of children in the respective families is nearly the same. We have done this as follows:

	Number of Families	Number of Children	Number of Children
Abstainers...	711	2855	4.01
Moderates ...	1210	6050	5.00
Drinkers ...	1799	8978	4.99

It will be seen that this does *not* make the number of children the same. As we also have shown in our memoir, the users of alcohol have more children. The total number is not 3611, but 3720, which indicates that Laitinen has added in the special 109 families of 50 abstainers and 59 drinkers, which contains no moderates and of which there is no evidence to show that the babies were weighed (see p. 265). But a graver point is: how was the mortality calculated? The schedule babies *must* have lived to eight months; there is no evidence that the author ever collected data later as to their mortality. If they are included in the living children, the mortalities calculated are wholly erroneous. Also the percentages of miscarriages given in the case of 5845 births, 2125 of which are cases of selected non-miscarriages, can only be fallacious. We have endeavoured by aid of the mean ages of mothers and fathers for the first and second series to deduce the mean ages of Laitinen's third series from his Table IV. We have obtained the following results:

	Mean Age of Mothers	Mean Age of Fathers	No. of Children	Ages at Marriage	
				M.	F.
Abstainers...	38.73	42.79	4.01	30.71	34.77
Moderates ...	34.90	41.11	5.00	24.90	31.11
Drinkers ...	36.25	41.58	4.99	26.27	31.60

Our probable age at marriage is calculated as before, allowing two years for each child. Perhaps more ought to be allowed because the ages of husband and wife at marriage, especially among the abstainers, are very high. No allowance has been made for this by Laitinen, and yet we see that the age of the mothers in the drinking groups is about five years less than the ages of those in the abstaining groups, while the age of the fathers is about three years less. It is quite certain that the age of the mother at least and probably the age of the father affects the well-being of the child, and that

the presence of many young mothers in the alcoholic group may influence the weight and general health of the child quite as much as the moderate drinking of the father. Until Laitinen's data are treated by more adequate statistical methods and allowance made for possible racial and parental age differences in the three groups, we cannot for a moment accept as final his conclusion that: "If we reflect upon the facts above mentioned, we find that all observations, whether made on a small or on a large scale point in the same direction, namely, that the alcohol drinking of parents, even in small quantities (about one glass of beer a day), has exercised a degenerative influence upon their offspring" (p. 269).

As for the facts themselves there is nothing which in the least refutes anything in our paper. We have noticed a differentiation in weight of an equally slight character, and a substantial difference in mortality. Only a person who has studied neither paper could assert that they do refute each other. But the statistical method of handling the facts and the sweeping and dogmatic conclusions drawn by Laitinen from them are unjustifiable. Laitinen's statistical method is often at fault; he has committed all the crimes of which we have been accused, but not convicted. And his populations have far more evidences of differentiation than ours exhibit. The high praise which Dr Saleeby* and Dr Johnston† bestow on this work is only evidence of their complete inability to weigh scientific investigations.

(4) *Bezzola's Memoir*. As a further exceedingly instructive illustration of the real value of the judgment of our medical critics we will take a paper by Bezzola‡. This paper, apart from a large amount of more or less irrelevant introductory talk, professes to examine a fairly simple statistical problem. The number of births in Switzerland, 934,619, for the years 1880—1890 are distributed in their respective months; then the births of 8,196 imbeciles are taken from a census issued in 1897 and dealing with the children who in the years 1880 to 1890 were incapable of going to school or of profiting by going to school. After omitting the epileptics, deaf-mutes, the blind, cripples and physically defectives, the moral degenerates, etc.—4287 in number, Bezzola distributes the remainder, 8196 (*Schwach- und Blödsinnige*), in the months of their birth. The statistical problem is then to ascertain whether this sample of 8196 differs in distribution significantly from the 934,619 total of all births. This total population Bezzola speaks of as the *normal* curve of distribution. It is of course the total birth population and contains the imbeciles as a contributory factor. Now here is a perfectly simple problem. We have to place those 934,619 individuals in 12 groups, and draw from this material a random sample of 8196 individuals. We have then to ask what will be the probable range of deviations

* He finds Laitinen's study "an admirable contrast in method (!) and manner" to our memoir (*British Journal of Inebriety*, Vol. VIII. p. 63).

† "Very different from it [our memoir, "discredited as a scientific pronouncement"] is the masterly work of Professor Taav Laitinen of Helsingfors" (*Alcohol, the Parent and the Child*, p. 18).

‡ Statistische Untersuchungen über die Rolle des Alkohols bei der Entstehung des originären Schwachsinn. Bericht über den VIII Internationalen Congress gegen den Alkoholismus abgehalten in Wien 14. April 1901, Leipzig u. Wien, 1902, S. 109—111. Almost verbatim also in *Internationale Monatsschrift zur Bekämpfung der Trinksitten*, 11. Jahrg. 1901, S. 171—183.

of any such random samples, and inquire where the actual sample of 8196 imbeciles exceeds these limits. Now does Bezzola attempt any scientific solution of this problem? Not in the least; he plots solely the *variations* in the monthly birth-rate of the whole population and of the imbeciles on a scale which hugely exaggerates the differences between the "normal" and the imbecile curves; and then whenever he finds a rather marked visible difference, he at once seeks about for some festival season nine months earlier to account for it on the prejudgment that alcohol at festivals is the source of imbecility in offspring. Not chronic alcoholism, be it noted, but acute alcoholism at the moment of conception.

In Diagram III we have endeavoured to reconstruct the data upon which Bezzola worked, distributing the 8196 imbeciles into their respective months. The polygon *AA* gives a "normal" population of 8196 individuals, *i.e.* the total 934,619 reduced to this total. On either side of this polygon we have set up twice the probable error, and obtained the belt bounded by *CC* and *C'C'*. Within that belt, on the average for each month, nine out of ten random samples would fall. Hence in taking *twelve* months, we should expect at least one point on the random sample curve to fall outside this belt on the theory of pure chance selection of 8196 individuals out of 934,619. This is exactly what occurs in Bezzola's data. The imbecile curve lies in 11 months entirely within the zone of twice the probable error and then drops outside in one month, just as it should do if it were a purely random sample. In other words there is not one grain of evidence in Bezzola's data for a statistical differentiation in the periods of the year in which imbeciles and the general "normal" population are born.

Now if the reader will fix his attention on the diagram he will see three points on the *DD* or imbecile curve marked α , β and γ . The excess at β of the dotted line over the continuous line represents *three* births due to the October conceptions* in a total of

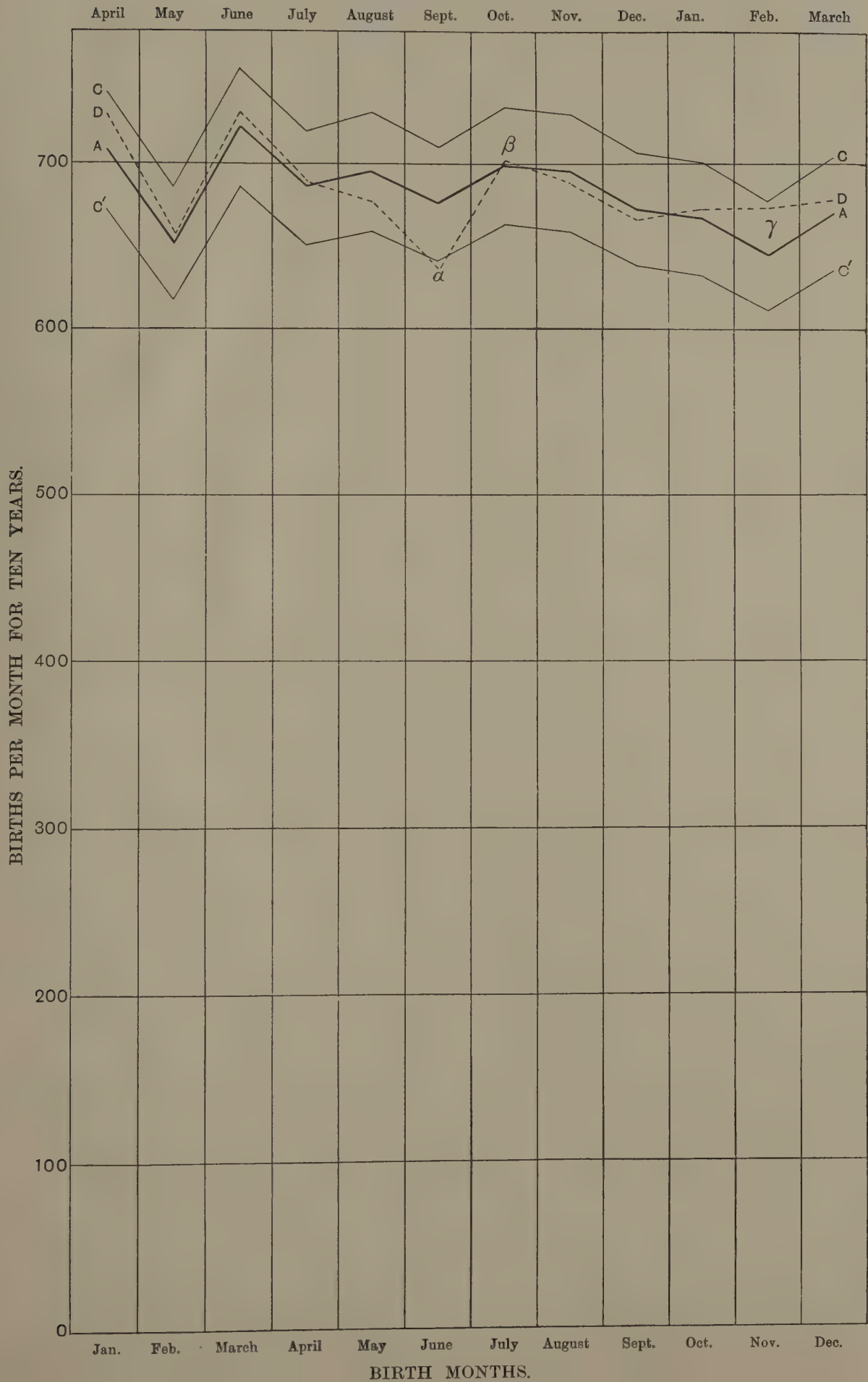
* The language in which Bezzola describes this deviation of *three* imbeciles from the expected number is so very characteristic of the method of writing adopted by temperance medical writers, that we cite it at length, having premised that it is the fall in September—were it really significant—and not the normal number of births of imbeciles in October, which would need explanation.

"Aber im Oktober flackert es plötzlich wieder auf. Man hat sich zu neuen Excessen erholt und die Weinlese in manchen Gegenden sorgt für neues Keimgift. Ich betone, dass diese aus der Tiefe des Septemberstandes herauf plötzlich wieder aufsteigende Schwachsinn-Zeugungscurve zu einer ganz ungeahnten Höhe ansteigen würde, wenn die Schweiz nicht nur zum gerinsten Teil Weinland wäre. Ein Beweise dafür bietet der Kanton Waadt, wo in diesem Monat die Verhältnisszahl 112 erreicht wird und das Weinjahr 1885, dessen gesegneter Monat Oktober bewirkte, dass die tägliche Durchschnittszahl der Geburten Schwachsinniger im Juli 1886 die stattliche Nummer 209 annimmt, also mehr als das Doppelte des Tagesdurchschnitts des ganzen Jahres erreichte" (*loc. cit.* S. 182).

Bezzola tells us that the population—presumably the "Sommerfrischler"—have refreshed themselves for new excesses, and that the vintage in many districts provides for new poison, this to explain an absolutely non-significant addition of less than 0.5 % to the imbecile birth-rate calculated on the normal birth-rate for the month! He apparently sees the absurdity of this and informs us that this October excess (!) would reach an astonishing height if Switzerland were not to the smallest extent a wine country. He seems to forget that he has just told us that the height reached is due to the vintage in *many* districts! To demonstrate the effect of the vintage, which is absolutely imperceptible on his own statistics, and to provide a proof of his assertion Bezzola then proceeds to quote what appear gigantic numbers for the

DIAGRAM III.

CONCEPTION MONTHS.



700. This is *absolutely without significance*, but it is the "fact" on which the absurd talk about the influence of the vintage on the conception of imbeciles is based. At γ we have an excess of 31 births or a deviation of 4.8 per cent.; this deviation, as the diagram shows, lies well within the limits of a random sample, yet Bezzola associates it with drinking at the Shrove-tide festival in February*! Christmas and New Year festivals and the harvest festivals, to say nothing of Walpurgisnacht—a far more sexual festival than many others in many Teutonic districts—produce not even the slightest effect on the imbecile curve as distinct from the "normal" curve. But there is a drop greater than any other deviation among the conceptions of imbeciles in September. As we have already seen, *one* such deviation was to be expected. But not even our statistical tyros could overlook a difference of 40 births when they were emphasizing differences of 31 and of 3. How does Bezzola explain the drop in August Canton Vaud. He says that in the October of the wine-year 1885 the *daily* average of imbecile conceptions reached 209, or more than double the daily average of the whole year. The unsuspecting layman at once supposes that 109 extra imbeciles per day were born in the Canton Vaud owing to the vintage of 1885! Of course Bezzola does not give his absolute numbers, or his argument would crumble in the stating of it. But we have enough data to get out something like his original numbers, and show the absurdity of his whole reasoning. 8196 imbeciles were born in ten years in Switzerland, or approximately 68.3 per month. The population of the Canton Vaud is .085 of the whole population of Switzerland, thus Vaud must contribute 5.8 imbeciles per month to the births of imbeciles. Therefore all Bezzola means when he says that the *daily* average of imbecile conceptions rose from 100 to 209 in the October of the wine-year 1885 is that in the month in question 11 or 12 imbeciles instead of the average six were born, *i.e.* this vintage, if it contributed anything at all, contributed not 109 imbeciles per day but a total addition of six during the vintage. But he never stays to ask whether this number has any real significance. The actual problem to be answered is this: If on an average 5.8 imbeciles per month are born, what is the chance that in ten years, *i.e.* 120 trials, we shall find a month with 12 imbecile births? The number of births in the Canton Vaud for the period in question must have been about 662 per month of each year, or the chance of an imbecile birth about $\frac{1}{114}$. Accordingly the distribution of random samples of imbecile births follows the binomial $(\frac{113}{114} + \frac{1}{114})^{662}$. The first 20 terms only are significant in at least the fifth decimal place, and the chance of 12 or more imbeciles being conceived in one month is .01564. In other words on the theory of random sampling we should anticipate that this excess of idiots would occur twice in the 120 months considered by Bezzola for the Canton Vaud! Bezzola found it *once*; there is nothing whatever of significance in that on the numbers cited. Add to this that had a like excess of imbecile conceptions occurred in December, it would have been put down to "Weihnacht," if in January to "Neujahrzeit," if in February to "Fastnachtzeit," if in April to "Maibowlen" at Easter, if in May or June to "Hochzeitsfesten," and so forth,—and we see how hopelessly absurd is this sort of reasoning. Yet such stuff, without any scientific value, is acclaimed by Dr Saleeby, Sir Victor Horsley and Dr Basil Price as worthy of credence, and Senator and Kaminer actually speak of it as a proof of a "characteristic and indisputable" kind that acute intoxication has an immediate toxic effect on the germ-cell! There is not the least evidence direct or indirect to show that the parents of the half-dozen extra imbeciles who were conceived during the month of October, 1885, in the Canton Vaud were ever acutely intoxicated in their lives, or were in 1885 or any other year even engaged in vintage operations. Select a big enough scale, however, and talk about a *daily* average of 100 imbecile conceptions being raised to 209 instead of giving the actual numbers of extra imbecile conceptions as half a dozen in the whole month, and this simple artifice of multiplying by 500 will succeed in impressing those members of the unscientific public who like voracious puppies swallow all they come across without nosing it.

* If it were significant, which it is *not*, it would be more reasonable to associate it with the possibility that mentally defective women were more likely to become mothers and therefore to produce defective children at these periods than to associate it with the alcohol drunk.

and September? "Die gesunde Arbeit der Landbevölkerung einerseits und die vernunftgemässe Lebensweise der Sommerfrischler lässt für Alkoholexcesse wenig Raum, auch sind die Festlichkeiten rarer geworden und die Vereinsmeierei macht Ferien."

Had August and September been months of excess of imbecile conceptions instead of defect, no doubt stress would have been laid on the *Aerntegebräuche*, on *Maria Geburt* and *Maria Himmelfahrt*. Those who know the habits of the Teutonic peasantry well, and one of us has spent many months among them, know that alcohol on these occasions is consumed with a vigour as great, and that the festivities are as widespread, as at the vintage, which touches a far smaller range of the population. Meanwhile the "vernunftgemässe Lebensweise der Sommerfrischler" is an exquisite idea to account for a purely random drop in the number of imbeciles conceived in September. Had May shown a marked excess (it is actually as much in excess as the vintage month!) no doubt the trouble would have been attributed to the *Pfingst* drinking! Even the *Sommerfrischler* are too much for Leppmann, who states that Bezzola's results showed in July, August and September a "retrogression which affected principally the imbeciles-chart (plenty of work and few holidays)*." In other words, one of these doughty champions of degeneracy following even temporary consumption of alcohol finds that holidays diminish in the August and September period and argues for fewer imbeciles, the other attributes directly to the holidays being then in season the absence of imbeciles. Neither explanation is of any importance, as we have already indicated.

Now let us see what our own critics have to say about Bezzola. First and foremost for rashly accepting results without weighing them must always be taken, Dr Saleeby. This is what he writes (*The Eugenics Review*, Vol. II. p. 41): "Reference must also be made to the well-known work of Bezzola, which may be most conveniently (!) quoted from Forel as follows":

"The recent researches of Bezzola seem to prove that the old belief in the bad quality of children conceived during drunkenness is not without foundation. Relying on the Swiss census of 1900, in which there figure nine thousand idiots, and after careful examination of the bulletins concerning them, this author has proved that there are two acute annual maximum periods for the conception of idiots (calculated from nine months before birth). In the wine-growing districts the maximum conception of idiots at the time of the vintage is enormous, while it is almost *nil* at other periods. Moreover, these two maximum periods come at the time of year when conception is at a minimum among the rest of the population; the maximum of normal conceptions occurring at the beginning of summer."

Forel then goes on upon the basis of this slender evidence to talk about the germ-cell leaving its gland at the moment when it is impregnated with alcohol and the resulting individual developing all kinds of taints and defects. This may or may not be true; all we can say is that the imbecile births diagram provides no evidence whatever for the state of affairs described. Let the reader fix his eyes on the points β and γ of our chart, and consider whether the deviations from the normal justify

* Leppmann in Senator and Kaminer's *Health and Disease in Relation to Marriage*, Vol. III. p. 1092.

a single line of Forel's statement. It is only reasonable to suppose that Dr Saleeby has never seen Bezzola's "well-known work" which he finds "most conveniently quoted from Forel." Had he done so he would have known that (i) the data were not taken from the Swiss census of 1900, (ii) there were nearer eight than nine thousand imbeciles, (iii) there are not two *acute* annual maximum periods for the conception of idiots, (iv) there is no evidence for the conception of idiots at the time of the vintage being enormous and almost *nil* at other periods in either the wine-growing districts or any others*, (v) the two periods of maximum conception of idiots do not coincide with the times of the year when conception is at a minimum in the rest of the population. In fact no statement made by Forel in the extract cited by Dr Saleeby is correct†. But we have not done with Dr Saleeby yet! In his paper in the *British Journal of Inebriety*, Vol. VIII. p. 59, he tells us: "We have Bezzola's inquiry showing that in Switzerland most idiots are conceived at the time of the vintage." Will the reader again fix his attention on our diagram and note how the excess of *three* idiots, lying in itself well within the limits of random sampling (see the point β and carry the eye to the bottom of our diagram!), has now been magnified into "most idiots"?

Dr Basil Price—honorary treasurer of a society which professes to *study inebriety*—finds the memoir of the present writers "effectively criticized" by an author who can make such statements as we have just cited above. He himself contributes to the humour of the situation the following paragraph (*British Journal of Inebriety*, Vol. VIII. p. 73):

"In certain wine-growing districts of Austria it has been shown that the majority of imbeciles are conceived during the periods when most drinking takes place."

Here we have a perfectly definite statement as to a *majority of imbeciles being conceived*, although it is delightfully vague as to what the periods in question are. This paragraph has been taken as confirming Bezzola's results (?) as to the vintage. On what is it based? Dr Basil Price merely cites Leppmann in Senator and Kaminer *without reference to a page*. The only passage we can find bearing on the subject in Leppmann's paper occurs on page 1092, and runs: "At the discussion on this interesting communication (*i.e.* Bezzola's) at the Vienna Congress against alcoholism, a medical man said that the teachers in wine-growing districts of Lower Austria know that a material of very bad scholars in any one year denotes a good vintage 6 years previously."

This statement made by a nameless man‡ as to the opinion of teachers as to

* The argument as to the vintage would be almost exactly paralleled if September were shown to be a month with an excess of imbecile births in Kent, and it was then attributed to beer, because it was the month of the hop-harvest. Are the "Weinleser" supposed to consume the grapes or the must?

† Yet Dr Saleeby writes: "The well-known research of Bezzola...was based upon an enormous number of cases derived from official statistics. The evidence has been studied and accepted by such a careful critic as Professor Forel, and the precise data for which Dr Ryle asks [evidence of the results of conception during acute alcoholism], as if we had them not, are the very data the careful analysis of which led Bezzola to his conclusion" (*National Temperance Quarterly*, Sept. 1910, p. 170).

‡ It is actually quoted again as scientific evidence by Sir Victor Horsley (*Alcohol and the Human Body*, p. 326).

bad scholars for the whole of one year—an absolutely worthless statement for any scientific purpose, with not a number in it nor a measure of “bad” and “good”—is directly modified by Dr Price into an assertion that *it has been shown, i.e. demonstrated*, that a *majority* of *imbeciles* are conceived in Austrian wine-growing districts during the periods when most drinking takes place! Can any better proof be given that the foremost medical champions of degeneracy as a product of alcohol in the parent at the time of conception are entirely untrustworthy as critics and as men of science? But there is a more serious aspect to the whole matter. The *British Journal of Inebriety* is the organ of the Society for the Study of Inebriety, and on the first page of this *Journal* is a list of the vice-presidents of this society. It includes among other well-known names those of Sir Clifford Allbutt, Professor William Osler, Dr F. W. Mott and Dr R. Welsh Branthwaite, men who certainly weigh their arguments before they speak. Have these leaders of medical thought any idea of the gross absurdities to which they, unconsciously perhaps, but none the less effectively, are giving currency by the appearance of their names in this *Journal*? We had no desire whatever to examine the literature of this subject *before* starting our own investigation; we determined to come unprejudiced to an examination of the problem on unbiased data. This we accomplished, and we published our results indicating that the consumption of alcohol in the parent did not produce any *marked* mental or physical degeneracy in the offspring of school age. We have been assailed by a perfect army of critics—largely furnished with old-fashioned blunderbusses—who apparently confuse the whole alcohol problem with the problem of the influence of parental drinking on the mentality and physique of the offspring. They sympathise with Laitinen when he asserts that one glass of beer a day exercises a degenerative influence on the offspring or with Bezzola when he anticipates that: “Man wird vielleicht einmal zur Einsicht gelangen, dass jeder Tropfen Alkohol beim Erzeuger einen Tropfen Dummheit bei Erzeugten bedeutet” (*Internationale Monatsschrift zur Bekämpfung der Trinksitten*, 1901, p. 183).

These extreme views may or may not be ultimately demonstrated to be correct. We are quite certain that at present there is no definite evidence at all in their favour. We are now also fairly convinced that evidence collected by such critics as we have referred to will never be treated by any scientific method that we could trust, and that we should further have to ask overwhelming proof of the unbiased nature of its origin. We believe we have sufficiently illustrated in our sample of three stock temperance medical memoirs how wholly lacking these writers are in critical instinct; any processes or any data are sufficient, if they can be made to support a preconceived opinion*. Such authors can on one page speak of the “thorough

* Thus Sir Victor Horsley tells us that we have wholly neglected “Galton’s well-known law that the contribution of the parents to the physique and mentality of a child is about one-half of the whole, that of the grandparents one-eighth, and so on. When one parent only is considered (see memoir) this gives us only one-fourth of the total hereditary forces at work, and consequently no conclusions whatever ought to have been drawn” (*National Temperance Quarterly*, Sept. 1910, p. 181). Now Sir Victor Horsley is one of the persons who assert that alcohol produces a toxic effect on the germ and so on the offspring. What has this to do with Galton’s law, which, whether true or not, was stated by its author as

investigation" of Dr MacNicholl, and the next page state that the absence of any inquiry as to the "habits of the parents *previous* to and *during* conception of the children" quite vitiates any value in a report (Dr Basil Price, *N. T. Q. loc. cit.*, pp. 176—7). Such authors can in one breath tell us that although we cannot for obvious reasons find out the exact condition of the parents at the conception it is possible to find out "their habits of life," and yet in the next assert that the "habits of life" are not sufficient but that we require knowledge of three or four generations of the stock! In short everywhere logical confusion, chaos and misstatement.

One remarkable point remains to be noticed. In April 1901 a Report was presented to the "Society for the Study of Inebriety" by their Committee on Heredity consisting of nine medical men. This Committee state that:

"They are aware of and have devoted full consideration to the widespread belief that parental indulgence tends to render the offspring more innately prone than they otherwise would have been to excessive indulgence, but they can only reiterate their conviction that the existing evidence on the subject does not at present warrant such a conclusion" (Art. XIV). And again:

"In particular there is no evidence that characters acquired by the parent through indulgence in drink are inherited by the children subsequently born. The committee are aware that it is possible that the mental and physical states produced

applying to an inherent, not acquired, germinal character? The answer to this question is at once found by examining Sir Victor Horsley's book on *Alcohol and the Human Body*. He does not know what heredity means in any modern scientific sense. He speaks of "the appalling force of hereditary influence" when he is referring indifferently to the toxic effect of alcohol on the germ, or to an inherent character of the germ-plasm of a given stock. When Dr Crothers says that in reporting 1744 cases of inebriety he found 1080 with a distinct history of heredity, it never appears to occur to Sir Victor that the case may not be the same as that of alcoholised hens' eggs or the puppies of alcoholised dogs or guinea-pigs. Fancy a modern scientist talking about the manner in which "the appalling force of hereditary influence may be mitigated!" The only manner in which you can mitigate heredity is to cease to breed from bad stock, and no toxic effect of either alcohol or syphilis has anything to do with true heredity. As for Sir Victor Horsley's statement that no conclusions whatever ought to have been drawn from our memoir "because when one parent only is considered only one-fourth of the total hereditary forces are at work," the reply is obvious that if one-fourth produces zero effect, four times one-fourth will also produce zero effect. The application of the law to such a case, however, is wholly meaningless. Sir Victor Horsley's final conclusion is one of the most sweeping that we have yet come across! "The fact is there is only one way in which this question can be properly studied, and that is by obtaining data from some source which can provide instances of genuinely abstaining families for three or four generations. These should be compared with people in similar circumstances of life, amongst whom it can be proved that drinking habits have prevailed for the same period" (*loc. cit.* p. 181). Excellent doctrine! We need data covering 75 years or a century of families remaining in the same circumstances. But until that material is forthcoming, why does Sir Victor Horsley write a book on *Alcohol and the Human Body*, why does he write a chapter on Parental Alcoholism, why does he open it with a quotation that "Hereditary alcoholism is an undeniable fact," and why, above all, does he cite Laitinen, MacNicholl, Crothers and Bunge, who have wholly failed to comply with the "only" way "in which the question can be properly studied" as authorities worthy of consideration in the matter? There is only one answer to these questions. This criticism was not part of his intellectual stock-in-trade, or he would have applied it *ab initio* to these investigations. He ran up against it as a sort of weapon which might possibly fit the case when he found facts not in accordance with his own preconceived opinions.

in the parent by indulgence in alcohol do affect the child in some way through inheritance; again, they admit as possible, though strictly speaking this is no question of the inheritance of an acquirement, that indulgence may so damage the parental tissues that the germ is ill-nourished and the child is thus affected; yet again they admit as possible that the alcohol circulating in the parents' blood may directly affect the germ and in this manner affect the offspring as by producing degeneracy. *But these speculations have not been strongly supported by any evidence tendered to the Committee*" (Art. VI).

The words we have italicised hold as truly in 1910 as in 1901, there is no strong support in any evidence yet produced for these speculations. Yet when we come to the same conclusions as the Committee on Heredity of the Society for the Study of Inebriety, on the basis of quite independent and unbiased material, the *Journal* of that Society has not invective enough at its disposal to describe our "mischievous and indefensible contribution to the printed folly of nations." To such rhetoric we can but reply in the words of the old song:

Pray, Goody, please to moderate the rancour of your tongue,
Why flash those sparks of fury from your eyes?
Remember when the judgment's weak, the prejudice is strong.

(5) *Demme's Contribution to the Subject.* We will now pass to a last illustration of the manner in which our medical critics deal with statistics and the amount of care they give to their critical examination. We will consider the results of Demme. Horsley quotes Demme from Hodge, Basil Price quotes him from Kirby who again quotes him without any reference to the original at all (*British Journal of Inebriety*, Vol. VI. p. 166). The tables given by Horsley and Basil Price are not in agreement. With such a method of quoting authorities can we wonder if the title "scientific" be denied to papers published by these temperance writers? It almost compels one to believe that they have not themselves studied, and do not want others to study, the originals from which their data profess to be drawn. We shall content ourselves by criticising the data of Demme as they are actually cited without comment or warning by Sir Victor Horsley and Dr Basil Price.

The following is the record of the children of 10 drunkards and 10 sober pairs of parents prepared by Professor Demme and provided by Dr Basil Price:

Children of Sober Parents				Children of Drunkards			
			Totals				Totals
Entirely normal	50	Entirely normal	9
Mentally feeble, by no means idiotic	2	Idiots	8
Died of general weakness	3	Had epileptic or convulsive fits	13
Died of gastric catarrh	2	Became drunkards, with complication of			
Had chorea	2	epilepsy and chorea	5
Physical deformity	2	Deaf mutes	2
				Physical deformity	3
				Dwarfs	5
				Died in early infancy	12
			Total 61				Total 57
							4—2

Now either these 10 cases of sobriety and 10 cases of inebriety are typical of the whole population of sober and drunken parents or they are not. If they are *selected* cases then Sir Victor Horsley and Dr Basil Price have no logic whatever when they cite them as exhibiting the influence of alcohol in the parent. We must start therefore with the assumption that they represent the average results of sobriety and inebriety*. Let us see whither it leads us. The percentage of drinking parents in Switzerland must be at least 30%. This is, as the American and English returns show, quite an average percentage, and Professor Demme himself tells us that 10% of the population of Berne *die* of alcoholism. (For Switzerland 10.5% of men from 20 to 40 years of age and 15.5% of men from 40 to 60 years are said directly or indirectly to have died of drink in the 15 greater towns between 1891 and 1899 †.) We can now reconstruct our total population of which our 10 sober people and 10 drunken people are supposed to be random samples. These random samples of parents would produce offspring in the proportions given in the table below. Now is this not another striking instance of the manner in which these gentlemen in the name of science thrust statistics on the untrained public without having the inclination or capacity to *weigh* them as well as cite them? In the first place we have an infantile death-rate which is wholly out of keeping with any vital statistics with which we are familiar. In the next place the drunkards have practically died out in the next generation, from some percentage like 30 they have dwindled to 2.5%! And if it be argued that we have taken too high a percentage of drunkards for Demme's district, then be it noted these results will be not bettered, but rendered far more anomalous! Make your drunkards only 10% of the population—an obvious under-estimate, because Demme tells us that 10% *die* of alcoholism,

Reconstructed Population according to Demme's Samples.

Normal Individuals	63.0%
Deaths in Infancy...	11.9%
Idiots and Mentally Feeble	6.4%
Epileptic	6.5%
Epileptic Drunkards	2.5%
Deaf-Mutes	1.0%
Cases of Chorea	2.3%
Dwarfs	2.5%
Physically Deformed	3.8%
Total				99.9%

* This is a very big assumption, as any statistician would have informed these gentlemen. The probable errors of the results based upon these numbers are enormous. For example, the probable error of 2 deaf-mutes occurring in 57 persons is .94, thus the real number of deaf-mutes might easily have been either 0 or 4; for 5 dwarfs in 57 it is 1.5, so that the number of dwarfs might have been easily either 2 or 8. Beyond this, however, we know that deaf-mutism and dwarfism run in families, so that these probable errors ought not to be based even on 57 units!

† *Archiv für Rassen- und Gesellschafts-Biologie*, Bd. I. S. 238. Dr Basil Price (*British Journal of Inebriety*, Vol. VIII. p. 77) asserts that on the most conservative estimate the alcoholic *death-rate* in England and Wales is 14%; this can hardly denote less than 30% who drink considerably.

and this must mean a far higher percentage of living alcoholists—and we find an infantile death-rate of 9.4% and a percentage of drunkards in the offspring population of 0.8%. We need to raise, not lower, our percentage of drinkers to make Demme's numbers agree with well-tested returns for infantile mortality. But let us look further. According to Demme's results the drunkards would practically die out in one generation. Their numbers would sink from 30% to 2.5%, or from 10% to 0.8% according to the hypothesis made as to the proportion of sober parents. Why, on Demme's figures drunkenness would practically cure itself in one generation, for the sober contribute nothing to the next generation of drunkards and the drunken parents only one-twelfth of their own numbers! This solution of the drink problem has never apparently occurred to Sir Victor Horsley, Dr Basil Price, or Dr Saleeby, when they cite such data as representing the average effects of sobriety and inebriety. What is absolutely necessary on the basis of Demme's figures, unless the sober are reduced to less than a moiety of the population, is that a large number of the normal offspring of the sober parents should become drunkards; there are not enough offspring left of the drinking parents to keep up the supply of drunkards. If the retort be made that the normal offspring of the sober were judged *as children*, then the whole card-house falls to the ground for Demme has entered "drunkards" in the offspring of the inebriate, but not of the sober—the two classes cease to be comparable. But what would happen if Demme reduced his sober parents to less than a moiety of the population? Why the numbers of physically and mentally defective in his sample—already preposterous to any one acquainted with the statistics of the subject—would become so obviously absurd that not even a temperance advocate would venture to cite them. Look at them as they stand. We have here a population with 9% of epileptics, with more than 2.3% of cases of chorea*, with 6.4% of idiots and mentally defective, with 1% of deaf-mutes and 2.5% of dwarfs.

In the general population epilepsy averages below 0.5% (0.03% in Switzerland), imbecility and mental defect below 2%, the prevalence of deaf-mutism is .07% in Europe (0.1% in Switzerland), and as for "dwarfs," can Dr Basil Price or Sir Victor Horsley have any idea when they cite such statistics of the prevalence of either achondroplasia or of true dwarfism? 30% of such a population as the above would be in special schools, asylums or reformatories. It is quite safe to assert that at a maximum not 5% of any European population is under these conditions.

(6) *Mental Defect and Extreme Alcoholism*. Do we then accuse Professor Demme of manipulating his statistics? Not in the least. We can more than parallel them from the pedigrees of robust and degenerate families in the possession of the Eugenics Laboratory†. We assert that on the face of it such cases are

* Some of the epileptic drunkards suffered from chorea.

† Quite similar groups of families are given by MacNicholl (*Journal of the American Medical Association*, Vol. 48, p. 398, 1907), who states that of 102 children in 25 families of heavy-drinking parents, seven had tuberculosis, eight had disease of the heart, 31 functional diseases of the nervous system, 41 were drinkers, six were degenerates and four were idiots; only five of the entire number were

demonstrably not *random* or average data concerning the offspring of sober and drinking parents respectively. Epilepsy, mental defect, deaf-mutism and dwarfism, when this term is used in any proper sense, are hereditary characters. But as our material increases it becomes more and more evident that there is a link between these various physical and mental defects; they occur more often in association than is reasonable on any basis of probability. Superficially and for the time being only we may possibly look upon it as the inheritance of some defect in a general development-controlling determinant. The idea of stocks exhibiting "general degeneracy" is not an idle one, if by general degeneracy we refer to the correlated appearance of improbable mental and physical defects in a group of blood relations. It is easy to obtain pedigrees of such stocks when the material to be drawn from is sufficiently large or itself perhaps already selected as in special schools, hospital practice and inebriate reformatories. In such stocks mental defect is almost always one of the correlated conditions, and where mental defect occurs there cases of extreme alcoholism also occur. In a paper shortly to be issued by this Laboratory, it will be indicated that the mental defect to be found associated with the *extreme* cases of alcoholism antedates the alcoholism, and is the antecedent not the consequent of it. Where there is mental defect, say in about 1% of the population, there in adult life alcoholism, prostitution and crime almost invariably follow. But because mental defect is an antecedent of extreme cases of alcoholism, and the children of such alcoholists show mental defect and other correlated abnormalities, it is purely idle to speak of parental alcoholism as the source of defect in the children. Mental defect exists in about 1% of the population, 20% to 40% of parents, according to the locality and occupation, drink. A large percentage of those parents is by *inheritance* neither physically nor mentally inferior; as our Edinburgh data appear to show, they may be rather above the sober average. Judged by asylum and reformatory data, judged by special pedigrees such as we possess or Demme cites, there is a close association between alcoholism in parent and mental defect in the offspring. Judged by a general sample of the population this relationship is not evident. The explanation is a perfectly simple one; the individuals with hereditary mental and physical defect drift into extreme alcoholism, and pass into asylum and reformatory; it is their hereditary characters which produce not only the alcoholism, but the defective offspring. From this *association* of alcoholism, insanity and defective offspring, a confusion has arisen in the minds of many between association and causation. They have not troubled to investigate whether or no normal. As Dr E. S. Talbot ("Alcohol in its Relation to Degeneracy," *Journal of the American Medical Association*, Vol. 48, p. 399) says, that excess in alcohol frequently occurs in degenerate stocks is undeniable. The discrepancies, however, between such statistics and others are great. "Lack of analytic skill and that dangerous, canting philanthropic tendency which rebels at statistics unfavourable to preconceived sociologic theories explain these discrepancies. The ignoring of all but the alcoholic factor produces also great elements of error. Kurnam (*Detroit Lancet*, Sept. 1882) cites 23 cases in which degenerate stocks were charged to alcoholic parentage, but which on analysis proved due to a degenerative factor in the parents of which alcoholism was merely an expression."

the mental defect antedated the alcoholism. The confusion at this point is so great that we must spend some time in driving the matter home. It is perfectly idle to quote statistics of the number of the insane or of imbeciles who have drinking parents, if at the same time the numbers of normal individuals who have drinking parents are not cited. It is usually assumed that if drinking parents are discovered in 20% to 30% of cases of insanity that this is evidence of parental alcoholism producing insanity in the offspring. Here is a statement from Dr Johnston, truly characteristic of this line of argument (*Alcohol, the Parent and the Child*, p. 11): "We have a body of authoritative evidence to show that a considerable proportion of imbecility and insanity must also be laid to its charge. The alcoholic taint was found by Dr Kerlin in 38 per cent. of all the cases in the Pennsylvania Institute for Imbeciles. Dr Shuttleworth puts the figure at 13·2 per cent. out of 1200 cases in the Royal Albert Asylum, and Dr Fletcher Beach at 19·5—21·4, if grandparents be included—out of 1180 cases at the Darenth Schools for Imbecile Children; while Dr Clouston says that 'from 18 to 20 per cent. of all cases of mental disease, wholly or in part in both sexes may be put down to alcohol as a cause (*Clinical Lectures on Mental Disease*, 1904).'"

Now just place the data of Dr MacNicholl cited by Dr Johnston himself against the American data of Dr Kerlin:

Percentages of Alcoholic Parents.

MacNicholl (Family histories of 3711 children)				Kerlin			
General Population				Imbeciles			
Drinking Parents	73 %	Alcoholic Taint	38 %
Drinking Parents and Grandparents	68 %				

That is to say the only authority quoted by Dr Johnston from whose writings we could test the value of his statement as to the effect of "alcoholic taint" on offspring shows almost *twice* the amount of alcoholic taint in the general population that we find among the population of imbeciles. It may be said that Dr MacNicholl's and Dr Kerlin's standards were not the same, but Dr Kerlin's data are of no service whatever to prove Dr Johnston's points until he has provided the only comparative data by which the matter could be judged.

Next take the Royal Albert Asylum data. Dr Johnston gives no comparative data in this case at all. We will, however, provide them:

Lancashire Manufacturing Town	Shuttleworth
Parents of children born in one year: "Notorious" drinkers 16 %	Imbeciles (Lancashire) Alcoholic Taint 13·2 %

What is the value of Dr Johnston's 13·2% without any comparative data?

The values given for the Darenth schools are wholly idle for Dr Johnston's purposes. So is most of the material from the lunatic asylums, which attributes 20%—30% of the insane cases to alcoholism as a primary or secondary cause. In the class from which public asylums and these schools draw their material

at a minimum 20% and a maximum 40% of the males in either generation, parent or offspring, use more alcohol than is good for them. The mere association of alcoholism with insanity either directly or through the parent to the extent of 30% of the cases is no evidence at all even of a special association, still less of a causation. The statements made by Sir Victor Horsley and Dr Johnston may in the sequel prove to be either true or false; what is quite certain is that the evidence they bring to demonstrate these statements is wholly valueless as it stands.

We will take one more illustration of this, namely from Sir Victor Horsley. He extracts a pedigree of insanity from a paper by Dr Mott (*British Medical Journal*, Oct. 28, 1905). The family consisted of three sons and five daughters. Of these, two sons and three daughters, most of them more than once, had been in asylums. Of the mother we are simply told "no history of insanity in family." Of the father "No family history of insanity, fits or nervous disease. Chronic drunkard from boyhood." He had been twice in an asylum. "Facts like these can only be explained," writes Sir Victor Horsley, "by admitting that the condition of the health of the father has a marked influence on that of his offspring" (*Alcohol and the Human Body*, p. 316).

Now we have had experience of very large numbers of pedigrees in the Eugenics Laboratory, and especially of the difficulties of ascertaining facts with regard to the class of people who go into public asylums. The first question we naturally ask is: Who gave the information about the ancestry of a man who was born in 1830? Has this "chronic drunkard from boyhood" been trusted to state what his parents and grandparents, his uncles and his aunts and his cousins suffered or died from? It is the most difficult thing to prove a *negative* in a case like this—yet "no family history of insanity, fits or nervous disease" is dogmatically stated by Sir Victor Horsley. Can he ever have studied family history in pedigree form? If he has, he would have learnt to distrust every pedigree which asserts negative heredity, and does not give *detailed* information as to the members of the ancestry. We have pedigrees of special defects which for five or six generations show no case of the defect, but coming across the same peculiar defect in other stocks we have been able to link them up to a common ancestor. No student of heredity, we feel certain, would accept a pedigree which simply states at the *parental* generation "no family history," and gives no details of collaterals or ancestry, and would argue upon it as evidence of a general principle that the alcoholism of the father had produced insanity in himself and the children. The facts that insanity existed in both generations, and that the father was a "chronic drunkard from *boyhood*," point to other factors which destroy any validity in the argument. Even Sir Victor Horsley gives away his own case for he admits: "Possibly there may have been a strain of initial mental defect in the father, which when transmitted, was increased by the poisonous action of the alcohol."

"Possibly" indeed! But where is the evidence that the strain of initial mental

defect was not sufficient without the "poisonous action of the alcohol" to produce the observed results? Alcohol produces insanity, therefore it is poisonous, but insanity being there it must be a product of the "poisonous alcohol": such is the vicious circle in which minds which rush to premature judgments move and have their being.

We are glad to notice that Dr Clouston, in his recent evidence before the Royal Commission on Divorce, is reaching what we think is daylight on this vital alcoholic problem. He is reported to have said:

"As to the number of the insane who might be said to have brought on their disease by their own acts, conduct and course of life, he said that at one time he made a careful inquiry on this point, and his conclusion was that in not more than one-third of all the persons who became insane could the disease be attributed to their own acts or course of life. Excess in use of alcohol, the syphilitic poison, and dissipated courses of life he found were the three chief causes, but there was an evident fallacy in coming to any certain conclusion on this important question. It consisted in this, that the mental heredity was so much stronger in some cases than others, that there were many people in whose cases a very little alcohol or dissipation would upset their mental working, and in many of them it was certain that if such causes had not been brought into operation in their youth they would have become insane all the same as the time went on" (*Times*, October 26, 1910, p. 4).

At the back of extreme alcoholic and sexual excess will be found almost invariably a want of will-power, of self-control, that is, a mental defect. At a maximum 5% of the population may be estimated to have been at one time or another certifiably insane. Dr Clouston's third of the insane who have brought insanity on themselves by their own acts is not very largely in excess of the known mentally defective persons in the community. What becomes of the 1% to 2% of mentally defective children in adult life? Are they or are they not the same mentally defective persons as we find in prisons, inebriate reformatories and asylums for the insane? If not, we ask again, what becomes of the bulk of them? If they are, then how idle must be the suggestion that alcohol *per se* produces mental defect, prostitution and crime. Until we know whether mental defect is the antecedent or consequent of extreme alcoholism, it is purely idle to heap up examples of the association of extreme alcoholism with defectiveness in the offspring. If mental defect be the antecedent and not the consequent, then because it is hereditary, it will always be possible to pick out defective offspring in some extreme cases of alcoholism. But this admitted association is no argument at all that alcohol in all forms leads to degenerate offspring. Put in the shape of a syllogism we state: Mentally defective parents have mentally defective offspring. Mentally defective persons are especially prone to alcoholism. Both these statements can be demonstrated, but the inference drawn by our critics that all indulgence in alcohol leads to mentally defective children is not legitimate, and is at present unproven. Yet it is precisely this inference which Sir Victor Horsley and his colleagues draw from such statements as those of Demme, Kerlin and Shuttleworth.

Nay, some recognised authorities go further. Professor Dr Ploetz—a man for whom we have a great personal regard—sees that Demme has really selected his material and taken extreme cases of good stock and bad stock, yet he goes on to say that between these extremes come the great population, who have control over themselves and make a moderate use of alcohol. He proceeds to argue that, if extreme alcoholism produces such effects, since all things are continuous in their effects lesser grades of alcoholism must also produce their proportionate quantum of defectiveness in the children*. He does not see that it is necessary first to demonstrate that the alcoholism is the antecedent and not the consequent of hereditary defectiveness in these extreme cases. There lies the problem of inebriety in its most fundamental phase†. And if that problem be answered in the way we believe it will be answered, namely, that there is an antecedent hereditary defectiveness in those cases where parental alcoholism has been found associated with mental and physical defectiveness in the offspring, on whose shoulders will the blame lie for the partial shipwreck of the temperance movement? Will it not certainly lie on the shoulders of the men who are forming premature judgments on great social problems without studying the weight of the evidence they deduce; who thrust on the public as proof data which are self-contradictory, and must sooner or later be admitted as such? The only way to produce lasting reform in social matters is to speak the truth and nothing but the truth to the man in the street. We doubt whether the public have been told the truth in this matter; they have been told what the supporters of the temperance movement honestly *believe*, but the moment the writings of these men are studied it will be seen that the fundamental problems have not yet been answered, and if answered in a sense different from the current belief, the whole basis of the temperance movement will be shaken. The fault of the consequent reaction in the public mind must lie with those who believe that prejudgments rather than continuous study will enable them to find the right solution for any social difficulty. Every social problem belongs to a class embracing the hardest of all problems—it is vital not physical, it is biological, it is medical, it is statistical. It needs not less but far more investigation for its solution than any academic physical or biological problem. Yet every politician, every platform orator, who would hesitate to express even his opinion regarding a question in astronomical physics or cytology is ready with a decisive answer to each social problem that arises. The staff of the Galton Laboratory naturally lays no claim to any special infallibility in either conclusion or choice of method, but it does assert and will continue to assert that these social problems, with their intense complexity, cannot be solved by political and oratorical methods; they must be answered as all other scientific problems by investigation of an academic kind in university laboratories.

* *Archiv für Rassen- und Gesellschafts-Biologie*, Bd. 1. S. 239.

† We might wish our temperance friends would take to heart the words of Edgar Allen Poe: "During the fits of alcoholic unconsciousness I drank—God knows how often or how much. As a matter of course, my friends referred the insanity to the drink, rather than the drink to the insanity."

Found such laboratories, provide them with the biological, medical and statistical equipment needful; create them in every university so that they may act as mutual checks! That is the manner in which physical and biological problems are solved, and nothing less will suffice in the case of these far harder problems. No solution is possible to men without academic leisure, without special training, who merely cite each other and weigh no evidence.

We have not discussed at length all the data provided by Sir Victor Horsley and his colleagues; we have merely sampled their material to indicate how little real knowledge flows from their methods of treatment. But if occasion arises we shall go further; our illustrations are not selected, they are a random sample of the "rebutting" evidence produced by the medical critics of our memoir. It is no discourtesy which prevents us from replying to the innumerable critics of our work in innumerable journals. We have thought it better to go to the core of the matter, and the nature of that core can be best illustrated in Sir Victor Horsley's method. It consists in mistaking occasional association for general causation; once that error is realised, the work of coping with the problem of alcoholism will have to be started *de novo*. It will be a national misfortune if temperance associations, societies for the study of inebriety, and popular writers on alcoholism pledge themselves to views which have no sound basis in observed facts, and expend their forces in invective rather than open-minded inquiry and sympathetic criticism.

